XXII. *Experiments relating to Phlogiſton, and the ſeeming Converſion of Water into Air. By* Joſeph Prieſtley, *LL. D. F. R. S.; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 26, 1783.

TO SIR JOSEPH BANKS, BART, P. R. S.

DEAR SIR,

AT the perſuaſion of my friends, I beg you would lay before the Royal Society my late obſervations on *Phlogiſton*, and alſo on the ſeeming *Converſion of water into air*, though I have by no means done all that I have in view with reſpect to theſe ſubjects. The principal *facts* are, I think, ſufficiently aſcertained, though I do not preſume to give any opinion with reſpect to the *theory* of them.

I am, with the greateſt reſpect, &c.

Birmingham, April 21, 1783.

Expe-

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Experiments relating to Phlogiſton.

THERE are few ſubjects, perhaps none, that have occaſioned more perplexity to chemiſts than that of *phlogiſton*, or, as it is ſometimes called, *the principle of inflammability*. It was the great diſcovery of STAHL, that this principle, whatever it be, is transferrable from one ſubſtance to another, how different ſoever in their other properties, ſuch as ſulphur, wood, and all the metals, and therefore is the ſame thing in them all. But what has given an air of myſtery to this ſubject, has been that it was imagined, that this principle, or ſubſtance, could not be exhibited except in combination with other ſubſtances, and could not be made to aſſume ſeparately either a fluid or ſolid form. It was alſo aſſerted by ſome, that phlogiſton was ſo far from adding to the weight of bodies, that the addition of it made them really lighter than they were before; on which account they choſe to call it *the principle of levity*. This opinion had great patrons.

Of late it has been the opinion of many celebrated chemiſts, Mr. LAVOISIER among others, that the whole doctrine of phlogiſton had been founded on miſtake, and that in all caſes in which it was thought that bodies parted with the principle of phlogiſton, they in fact loſt nothing, but on the contrary, acquired ſomething; and in moſt caſes an addition of ſome kind of air; that a metal, for inſtance, was not a combination of two things, *viz.* an earth and phlogiſton, but was probably a ſimple ſubſtance in its metallic ſtate; and that the calx is produced not by the loſs of phlogiſton, or of any thing elſe, but by the acquiſition of air.

The arguments in favour of this opinion, eſpecially thoſe which are drawn from the experiments of Mr. LAVOISIER made on mercury, are ſo ſpecious, that I own I was myſelf much inclined to adopt it. My friend Mr. KIRWAN, indeed, always held that phlogiſton was the ſame thing with inflammable air; and he has ſufficiently proved this from many experiments and obſervations, my own as well as thoſe of others. I did not, however, accede to it till I diſcovered it by direct experiments, made with general and indeterminate views, in order to aſcertain ſomething concerning a ſubject which had given myſelf and others ſo much trouble.

I began with repeating the experiments in which I had found that inflammable air, made red-hot in flint glaſs tubes, gave them a black tinge, and was in a great meaſure abſorbed, which I had diſcovered to be owing to the calx of lead in the glaſs attracting phlogiſton from the inflammable air. As the quantity of air in theſe tubes was very ſmall, though I gave it as my opinion, that the reſiduum in one of the proceſſes was phlogiſticated air, becauſe I perceived no marks of aſcenſion on preſenting to it the flame of a ſmall candle; I was not, on recollection, ſatisfied with this concluſion, and was deſirous of repeating the experiment with more care, eſpecially as, in one of the above-mentioned experiments, I found only a very ſmall bubble of the inflammable air in the tube in which it had been heated.

I found, however, great difficulties in repeating theſe experiments; and the quantity of inflammable air operated upon in them is neceſſarily ſo ſmall, that the reſult is always liable to much uncertainty. I thought, therefore, that throwing the focus of a burning lens upon a quantity of pounded flint glaſs, ſurrounded with inflammable air, or rather on the calx of lead alone, in the ſame circumſtances, would be a much eaſier experiment, and might bring me nearer to my object; and on making the experiment it immediately anſwered far beyond my expectation.

For this purpoſe, I put upon a piece of a broken crucible (which could yield no air) a quantity of minium, out of which all air had been extracted; and placing it upon a convenient ſtand, introduced it into a large receiver, filled with inflammable air, confined by water. As ſoon as the minium was dry, by means of the heat thrown upon it, I obſerved that it became black, and then ran in the form of perfect lead, at the ſame time that the air diminiſhed at a great rate, the water aſcending within the receiver. I viewed this proceſs with the moſt eager and pleaſing expectation of the reſult, having at that time no fixed opinion on the ſubject; and therefore I could not tell, except by actual trial, whether the air was decompoſing in the proceſs, ſo that ſome other kind of air would be left, or whether it would be abſorbed *in toto*. The former I thought the more probable, as if there was any ſuch thing as phlogiſton, inflammable air, I imagined, conſiſted of it, and ſomething elſe. However, I was then ſatisfied that it would be in my power to determine, in a very ſatisfactory manner, whether the phlogiſton in inflammable air had any *baſe* or not, and if it had, what that baſe was. For ſeeing the metal to be actually revived, and that in a conſiderable quantity, at the ſame time that the air was diminiſhed, I could not doubt but that the calx was actually imbibing ſomething from the air; and from its effects in making the calx into metal, it could be no other than that to which chemiſts had unanimouſly given the name of *phlogiſton*.

Before this firſt experiment was concluded, I perceived, that if the phlogiſton in inflammable air had any baſe, it muſt be very inconſiderable: for the proceſs went on till there was no more room to operate without endangering the receiver; and examining, with much anxiety, the air that remained, I found that it could not be diſtinguiſhed from that in which I began the experiment, which was air extracted from iron by oil of vitriol. I was therefore pretty well ſatisfied that this inflammable air could not contain any thing beſides phlogiſton; for at that time I reduced about 45 ounce meaſures of the air to five.

In order to aſcertain a fact of ſo much importance with the greateſt care, I afterwards carefully expelled from a quantity of minium all the phlogiſton, and every thing elſe that could have aſſumed the form of air, by giving it a red heat when mixed with ſpirit of nitre; and immediately uſing it in the manner mentioned above, I reduced 101 ounce meaſures of inflammable air to two. To judge of its degree of inflammability, I preſented the flame of a ſmall candle to the mouth of a phial filled with it, and obſerved, that it made thirteen ſeparate exploſions, though weak ones (ſtopping the mouth of the phial with my finger after each exploſion), when freſh made inflammable air, in the ſame circumſtances, made only fourteen exploſions, though ſtronger ones.

After this experiment I could not heſitate to conclude, that this inflammable air went totally, and without decompoſition, into the lead which I formed at that time; and if the neceſſary circumſtances of the experiment be conſidered, it will be thought extraordinary that, even admitting this, the reſult ſhould be ſo deciſively clear in favour of it: for, in the firſt place, the greateſt care muſt be uſed to expel all air from the minium,

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and it muſt be uſed before it can have atttacted any from the atmoſphere; and in the next place, the water alſo (a conſiderable quantity of which muſt be uſed, and which will alſo be heated in the proceſs) ſhould be made as free from air as poſſible. In theſe circumſtances, had I found the ſmall reſiduum, of 2 ounce meaſures from 101, to have been phlogiſticated or fixed air, I ſhould not have been diſappointed; and it would not have prevented my concluding that *phlogiſton* was the ſame thing with *inflammable air*, contained in a combined ſtate in metals, juſt as fixed air is contained in chalk and other calcareous ſubſtances; both being equally capable of being expelled again in the form of air.

Afterwards uſing a calx of lead, which had been prepared in the ſame manner with the former, but which had remained ſome weeks expoſed to the air, I found, that when by uſing it I had reduced 150 ounce meaſures of inflammable air to 10, this reſiduum was phlogiſticated air. But examining this calx ſeparately, I found that it gave, by heat in a glaſs veſſel, a conſiderable quantity of phlogiſticated air.

I muſt obſerve, that the minium ſhould not be reduced to a perfectly compact *glaſs of lead*; for then it would be too refractory to be eaſily revived by this proceſs. Making uſe of ſome of it, I found that I could only melt it; but that a copious black fume came from it, and coated the inſide of the receiver: an experiment which I ſhall repeat and re-conſider. I muſt alſo obſerve, that the lead which I procured in the above-mentioned proceſs was not to be diſtinguiſhed from any other lead, and that the inflammable air was all procured from iron by oil of vitriol.

When I made uſe of inflammable air from wood, I found, that though I was able to reduce minium with it, it was effected with more time and difficulty. Forty ounce meaſures of this kind of inflammable air I reduced to 25; when I found that the heat of the lens produced only *glaſs of lead*, and no *metal*. The air was ſtill, however, inflammable; and there was a ſmall mixture of fixed air in it. This kind of inflammable air, which burns with a lambent flame, I have ſome reaſon to think, conſiſts of an intimate union of fixed air with that which is of the *exploſive* kind extracted from metals. The reſult of thoſe experiments which I made with that kind of inflammable air which is collected in the proceſs for making phoſphorus, and which burns with a lambent yellow flame, was ſimilar to thoſe which I made with inflammable air from wood, which burns with a lambent white flame.

Having had this remarkable reſult with inflammable air, I immediately tried all the other kinds of air in the ſame manner; but in none of them did I procure any thing from the minium beſides glaſs of lead, except in alkaline air, and vitriolic acid air. In fixed air, nitrous air, phlogiſticated air, marine acid air, fluor acid air, as alſo in common and dephlogiſticated air, I got no metal at all. In vitriolic acid air there was but a ſmall quantity of lead produced, and I have obſerved that this kind of air imparts a certain portion of phlogiſton to common air, rendering it in ſome meaſure phlogiſticated, though by no means in ſo great a degree as nitrous air. Though nitrous air and phlogiſticated air certainly contain phlogiſton, they appear by theſe experiments to hold it too obſtinately to part with it to minium in this proceſs, though nitrous air quits it ſo readily to reſpirable air. I would obſerve, that there were ſome peculiar appearances in the experiments I made to revive the calx of lead in theſe kinds of air in which the attempt did

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not ſucceed; but I muſt repeat the experiments, and note the appearances more accurately, before I report them.

In alkaline air lead ſeems to be formed from the minium as readily as in inflammable air, and indeed I thought rather more ſo; and this is a remarkable confirmation and illuſtration of theſe experiments, in which, by taking the electric ſpark in a quantity of alkaline air, I converted it into three times as much pure inflammable air; an experiment which, on account of the extraordinary nature of it, I have repeated many times ſince I firſt publiſhed the account of it, and always with the ſame reſult.

This experiment alſo throws ſome light upon thoſe in which, by ſuper-phlogiſticating iron with nitrous air, I produced a ſtrong ſmell of volatile alkali; an experiment which I have alſo frequently repeated with the ſame reſult. The reviving of lead in alkaline air may alſo help us to conceive how all *acids* ſhould have an affinity both to *phlogiſton* and to *alkalies*, which have hitherto appeared to be things ſo very different from each other; ſince, from theſe experiments, it is probable that one of them is ſome modification of the other, or a combination of ſomething elſe with the other. To trace the connection between the alkaline and inflammable principles, is a curious ſubject; and from theſe hints it may, perhaps, not be very difficult to proſecute it to advantage. It is evident, however, from the following experiments, that alkaline air is the compound and inflammable air, or phlogiſton, the more ſimple ſubſtance of the two.

From 5½ ounce meaſures of alkaline air I got, by means of litharge, 17 grains of lead, beſides ſome that was diſſolved in the mercury, by which the air was confined. There remained 2½ ounce meaſures, which appeared to be phlogiſticated air, and to have no fixed air in it. At another time, in eight ounce meaſures of alkaline air I got 15 grains of lead, beſides what was diſſolved in the mercury, which ſeemed to be a good deal in proportion to it. It was obſervable, that there remained in this proceſs 3½ ounce meaſures of phlogiſticated without any mixture of fixed air in it, though the maſſicot which I uſed at this time gave by heat only a good deal of pretty pure fixed air. Theſe experiments with alkaline air well deſerve to be reſumed, and I ſhall not fail to do it at a proper opportunity.

Having thus produced lead in inflammable air, I proceeded in my attempts to revive other metals from their calces by the ſame means; and I ſucceeded very well with tin, biſmuth, and ſilver; tolerably well with copper, iron, and regulus of cobalt; but not at all with regulus of antimony, regulus of arſenic, zinc, or the metal of manganeſe.

I was deſirous alſo of aſcertaining by this means the *quantity* of phlogiſton that enters into the compoſition of the ſeveral metals; but in this I found more difficulty than I had expected: and this aroſe chiefly from the allowance that was to be made for the inflammable air which entered into that part of the calx which was only partially revived; and it was not eaſy to revive the whole of any quantity of calx completely.

After many trials, I think I may venture to ſay, that an ounce of *lead* abſorbs 100 ounce meaſures of inflammable air, or perhaps ſomething more; for in one reſult it ſeemed to have imbibed in the proportion of 108 ounce meaſures.

An ounce of *tin* abſorbs inflammable air in the proportion of 377 ounce meaſures to the ounce. An ounce of copper from verditer abſorbed 403 ounce meaſures; from a ſolution of blue vitriol, precipitated by ſalt of tartar, and afterwards made red-hot with ſpirit of nitre, 640; but from blue vitriol itſelf 909 ounce meaſures. In this caſe, however, much of the inflammable air went to the formation of the vitriolic acid air, the ſmell of which was very perceivable in the courſe of the experiment. The copper that I made in this way was brittle, and therefore ſeemed not to be perfectly metalized; but being fuzed with borax it became perfect copper, and, as I think, without any loſs of weight.

Biſmuth abſorbed inflammable air in the proportion of 185 ounce meaſures to the ounce. The calx I uſed was a precipitate from the ſolution of this metal in ſpirit of nitre.

Iron I got from a precipitate of a ſolution of green vitriol by ſalt of tartar, moiſtened with ſpirit of nitre, and expoſed to a red heat. This calx abſorbed in the proportion of 890 ounce meaſures of the inflammable air to an ounce of iron, which was in the form of a black powder; but to all appearance as much attracted by the magnet as iron filings. But it could not be expected, that perfect iron, containing its full proportion of phlogiſton, ſhould be produced in this manner, ſince inflammable air may be expelled from perfect iron in this very proceſs.

Silver I evidently revived from a ſolution of it in ſpirit of nitre precipitated by ſalt of tartar, and alſo from *luna cornea.* A quantity of this laſt ſubſtance abſorbed 23 ounce meaſures of inflammable air; but I could not get any calx of ſilver free from ſmall grains of the perfect metal, which was eaſily diſcovered by a magnifier, and therefore I could not aſcertain the quantity of inflammable air abſorbed by it.

Small grains of regulus *cobalt* I produced from zaffre, and inflammable air was abſorbed; but I did not eſtimate the quantity.

A quantity of *manganeſe* abſorbed 7 ounce meaſures of inflammable air; but I could not perceive any thing in it which had the appearance of metal. But I imagined I had not heat enough for the purpoſe, and mixing with it ſome calcined borax, I repeated the experiment, when there was again an evident abſorbtion of air, and in the courſe of that experiment, I once thought that I did perceive a ſmall globule of metal.

Zinc and *arſenic* were only ſublimed in this proceſs. The ſame was the caſe with the glaſs of *antimony*; but the experiment was attended with this peculiar circumſtance, that when the glaſs was melted in inflammable air it formed itſelf into needle-like cryſtals arranged in a very curious manner, though I could not produce that appearance in other kinds of air.

Inflammable air being clearly imbibed by the calces of metals, and thereby reviving them, is a ſufficient proof of its *containing* what has been called phlogiſton; and its being abſorbed by them *in toto*, without decompoſition, is a proof of its being nothing beſides *phlogiſton in the form of air*, unleſs there ſhould be ſomething ſolid depoſited from it at the ſame time that the proper phlogiſtic part of it was abſorbed. With reſpect to this, I can only ſay that, in the courſe of the experiments, I did not perceive any thing of the kind: for though in ſome of the proceſſes there was a black ſmoke produced, in others I could perceive nothing but part of the calx ſubliming, and clouding the glaſs. On this account, however, I could not pretend to aſcertain the weight of the inflammable air in the calx, ſo as to prove that it had acquired an addition of weight by being metallized, which I often attempted. But were it poſſible to procure a perfect calx, no part of which ſhould be ſublimed and diſperſed, by the heat neceſſarily to be made uſe of in the proceſs, I ſhould not doubt but that the quantity of inflammable air imbibed by it would ſufficiently add to its weight.

Beſides the formation of metals from their calces, I had other proofs, and of a nature ſufficiently curious, of inflammable air containing phlogiſton, though perhaps not ſufficiently concluſive with reſpect to its being wholly and ſimply phlogiſton itſelf. Thus, by means of it, I was able to make *phoſphorus*, *nitrous air*, *liver of ſulphur*, and *ſulphur* itſelf, in all of which phlogiſton is acknowledged to be a principal ingredient.

Throwing the focus of the lens upon a quantity of that glaſſy matter which is made from calcined bones by oil of vitriol in inflammable air, ſome of it was abſorbed, and all the inſide of the receiver was covered with an orange-coloured ſubſtance, which had a ſtrong ſmell of phoſphorus. I then wanted ſun-ſhine to continue the experiment; but I was ſatisfied that there was ſufficient proof of phoſphorus being actually formed in this manner. With alkaline air I ſucceeded much better.

In 2½ ounce meaſures of this air I produced, from the glaſſy matter mentioned above, 2 grains of phoſphorus in one maſs, the veſſel being only filled with white fumes during the proceſs. One-fourth of the bulk of the air remained, and this was inflammable, burning with a yellow lambent flame, exactly like that which is produced in the proceſs for making phoſphorus.

That nitrous air contains phlogiſton is ſufficiently evident, if there be any ſuch thing as phlogiſton: and I have farther proved, that it contains as much phlogiſton, in proportion to its bulk, as inflammable air itſelf. I have now, however, the farther ſatisfaction to be able to make nitrous air from its two conſtituent principles, *viz.* nitrous vapour and inflammable air. The moſt eaſy proceſs for this purpoſe is, to throw a ſtream of nitrous vapour into a large phial previouſly filled with inflammable air. In this manner nitrous air is inſtantly formed, and in great quantities; but as this nitrous vapour is produced by the rapid ſolution of biſmuth in ſpirit of nitre, which at the ſame time produces a quantity of nitrous air, the experiment is not quite unexceptionable. I therefore attempted the ſame thing in the following manner.

Taking a quantity of what I have called a *nitrated calx* of lead, which I firſt produced by uniting nitrous vapour to minium (in conſequence of which, from being a red and powdery ſubſtance, it became white, compact, and brittle), I placed it upon a ſtand, in a receiver filled with inflammable air, and throwing the focus of the lens upon it, there was a diminution of the inflammable air, which amounted to about two-thirds of the whole, and during this time lead was revived from the calx. After this there was no more diminution of the air, or revival of the calx: and then examining what remained of the air, I found it to be all ſtrongly nitrous: and, from the circumſtances in which it was produced, it muſt have been formed from the nitrous vapour contained in the calx, and the inflammable air in the receiver. In order to aſcertain the purity of this nitrous air, I mixed it with an equal quantity of common air, and found that they occupied the ſpace of 1,32 meaſures. Freſh nitrous air, made in the uſual way, and mixed with common air in the ſame proportion, occupied the ſpace of 1,26. This difference aroſe not from any impurity in the nitrous air, but from the mixture of the dephlogiſticated air, which is alſo expelled from this calx by heat.

Liver of ſulphur was procured by throwing the focus of the lens upon vitriolated tartar in inflammable air, and it appeared to be perfectly well formed.

Laſtly, to produce *ſulphur*, I threw the focus of the lens on a quantity of oil of vitriol, contained in an hollow earthen veſſel, and evaporated it to dryneſs in a receiver filled with inflammable air, in conſequence of which the inſide of the receiver acquired a whitiſh incruſtation, which when warmed had a ſtrong ſmell of ſulphur; and repeating the proceſs in the ſame receiver, I was able, this ſecond time, to ſcrape off enough of the matter to put on a piece of hot iron, and to produce the genuine blue flame, as well as the peculiar ſmell of ſulphur.

I ſhall conclude theſe obſervations on phlogiſton with two articles; one of which ſeems to contradict an eſtabliſhed maxim among chemiſts; and the other a former opinion of my own.

It is generally ſaid, that charcoal is indeſtructible, except by a red heat in contact with air. But I find that it is perfectly deſtructible, or decompoſed, *in vacuo*, and by the heat of a burning lens almoſt wholly converted into inflammable air; ſo that nothing remains beſides an exceedingly ſmall quantity of white aſhes, which are ſeldom viſible, except when, in very ſmall particles, they happen to croſs the ſun-beam, as they fly about within the receiver. It would be impoſſible to collect or weigh them; but, according to appearance, the aſhes thus produced from many pounds of wood could not be ſuppoſed to weigh a grain. The great weight of aſhes produced by burning wood in the open air ariſes from what is attracted by them from the air. The air which I get in this manner is wholly inflammable, without the leaſt particle of fixed air in it. But, in order to this, the charcoal muſt be perfectly well made, or with ſuch a heat as would expel all the fixed air which the wood contains; and it muſt be continued till it yield inflammable air only, which, in an earthen retort, is ſoon produced.

Wood, or charcoal, is even perfectly deſtructible, that is, reſolvable into inflammable air, in a good earthern retort, and a fire that would about melt iron. In theſe circumſtances, after all the fixed air had come over, I have ſeveral times continued the proceſs during a whole day, in all which time inflammable air has been produced equally, and without any appearance of a termination. Nor did I wonder at this, after ſeeing it wholly vaniſh into inflammable air *in vacuo*. A quantity of charcoal made from oak, and weighing about an ounce, generally gave me about five ounce meaſures of inflammable air in twelve minutes.

The ſecond article that I ſhall now mention affords an indiſputable proof of the generation of fixed air from dephlogiſticated air and phlogiſton, or inflammable air. I have ſeveral times given it as my opinion, that fixed air is a *factitious ſubſtance*, and a modification of the nitrous and vitriolic acids, my former experiments greatly favouring that concluſion; but that it was compoſed of dephlogiſticated air and phlogiſton, though maintained by my friend Mr. KIRWAN, I was far from being ſatisfied with, till I was forced to conſent to his proof of it from my own former experiments, and gave him leave to mention it, as he has done in his late excellent paper on ſalts. But I have lately had two direct proofs of it by experiment.

The firſt was when, in repeating a beautiful experiment firſt made by Dr. INGEN-HOUSZ, but with ſome variation, I was firing ſome ſhavings of iron in dephlogiſticated air confined by mercury, by means of a burning lens. In this way I quickly fired the iron, and it burned away in a very pleaſing manner. But what ſtruck me moſt was, that, of the air that remained, a conſiderable portion was fixed air, though in the receiver I had nothing but the pureſt dephlogiſticated air, together with the

iron,

iron, which could only give inflammable air. I would obſerve, that the melted iron formed itſelf into large balls, which appeared to be a mere *ſlag* or *glaſs*, and was no longer iron.

Afterwards, to put this hypotheſis concerning the conſtituent principles of fixed air to a more direct proof, I mixed iron filings, which gave only inflammable air, with red precipitate, which I found to give nothing but the pureſt dephlogiſticated; and when I heated them in a coated glaſs retort, they gave a great quantity of fixed air, in ſome portions of which nineteen-twentieths were abſorbed by lime-water; but the reſiduum was inflammable. However, when I mixed with iron filings a quantity of powdered charcoal, which I had found to give only inflammable air, the fixed air produced from it was ſo pure, that only one-fortieth part of it remained unabſorbed by water; ſo that this fixed air was as pure as that which is generally procured from chalk by oil of vitriol.

It appeared, in ſome of theſe experiments, that three ounce meaſures of dephlogiſticated air go into the compoſition of two ounce meaſures of fixed air. For one ounce of this red precipitate gave 60 ounce meaſures of dephlogiſtated air; and when mixed with two ounces of iron filings, it gave about 40 ounce meaſures of fixed air that were actually abſorbed by water, beſides a reſiduum that was inflammable. I had the ſame proportion when I uſed half an ounce of each of the materials. But when I uſed one ounce of each, I got only 20 ounce meaſures of fixed air, including the reſiduum. At other times I had different proportions with different quantities of iron filings and charcoal.

I cannot conclude theſe obſervations without taking notice, how very valuable an inſtrument in philoſophy is a good burning lens. This muſt have been perceived in many of my former experiments, but more eſpecially in theſe. By no other means can heat be given to ſubſtances *in vacuo*, or in any other kind of air beſides atmoſpherical; and without ſome method of doing this, no ſuch experiments as theſe can poſſibly be made. I therefore congratulate all the lovers of ſcience on the ſucceſsful attempt of Mr. PARKER to execute ſo capital an inſtrument as he has done of this kind. Such ſpirited and generous exertions reflect honour on himſelf and on our country. It is only to be wiſhed, that we could have lenſes of a ſmaller ſize (*viz.* from 12 to 18 inches diameter) made tolerably cheap, ſo that they might be in more common uſe. All my experiments were made with one of 12 inches in diameter.

Experiments relating to the ſeeming Converſion of Water into Air.

SINCE many perſons have expreſſed a wiſh to be acquainted with the experiments I have lately made, which at firſt ſeemed to favour the idea of a *converſion of water into air*, but which terminated in the diſcovery of a fact, in my opinion, ſtill more extraordinary, I ſhall ſubmit to the Royal Society the reſult of the obſervations I have already made; though, as yet, I have by no means been able to ſatisfy myſelf ſo fully as I could wiſh with reſpect to ſome particulars connected with the ſubject. All the *facts* which I ſhall ſtate may be depended upon; but it is probable, that different perſons may draw different *concluſions*

from

from them; and to mere opinions I have never ſhewn myſelf much attached.

Having formerly obſerved ſeveral remarkable changes in fluid ſubſtances, in conſequence of long expoſure to heat in glaſs veſſels hermetically ſealed (of which an account may be ſeen in the fourth volume of my Philoſophical Obſervations); I then formed a deſign of expoſing all kinds of ſolid ſubſtances to great heats, in a ſimilar ſtate of confinement; and for that purpoſe provided myſelf with a caſt-iron veſſel, which I could cloſe at one end, like a digeſter, and of ſuch a length, that one of the ends might be red-hot, while the other was ſufficiently cool to be handled. To this end there was a cock connected to a tube, by means of which I could let off ſteam, or air, in any period of the proceſs.

I imagined, that when ſubſtances conſiſting of parts ſo volatile as to fly off before they had attained any conſiderable degree of heat, in the uſual preſſure of the atmoſphere, were compelled to bear great heats under a greater preſſure, they might aſſume new forms, and undergo remarkable changes, ſimilar to what we may ſuppoſe to be the caſe within the bowels of the earth, where, by means of ſubterraneous fires, various ſubſtances bear great heats under very great preſſures.

I have had this inſtrument ſome years; but it was ſo ill conſtructed, that I could not make the uſe of it that I had originally intended. I therefore lately fitted up ſome gun-barrels in the ſame manner, and made my firſt experiment with limeſtone; expecting, that when the fixed air, and other volatile matters, that might be contained in it, ſhould be compelled to bear a red heat, without a poſſibility of making their eſcape, the ſubſtance itſelf might undergo ſome change; but I had no particular expectation concerning the nature of that change.

I had, however, been ſo often favoured with valuable reſults from merely putting things into new ſituations, that I was encouraged to make the experiment; but I found an unexpected difficulty in getting a cock that would be air-tight and ſteam-tight under ſo great a preſſure as I wiſhed to apply.

I was mentioning theſe ideas to Mr. WATT, in whoſe neighbourhood I have the happineſs to be ſituated, when he mentioned a ſimilar idea of his, *viz.* that of the poſſibility of the converſion of water, or ſteam, into permanent air; ſaying, that ſome appearances in the working of his fire-engine had led him to expect this. He thought that if ſteam could be made red-hot, ſo that all its latent heat ſhould be converted into ſenſible heat, either this or ſome other change would probably take place in its conſtitution. The idea was new to me, and led me to attend more particularly to my former projects of a ſimilar nature, and I began with lime-ſtone, wiſhing to try the effect of giving a red heat to lime in which water only ſhould be previouſly combined, thinking it might poſſibly have the ſame effect with making the water itſelf red-hot.

Accordingly, I took a quantity of well calcined lime, and mixing with it a little water, out of which all air had been carefully boiled, I expoſed it gradually to a ſtrong heat in an earthen retort, ſuch as I had been uſually ſupplied with by Mr. WEDGWOOD (who is as much diſtinguiſhed by his love and generous encouragement of ſcience, as he is by his improvements in his own curious art), not imagining that it could make any difference whether the lime, ſo prepared, ſhould receive its heat in an earthen retort, or in a veſſel of iron or glaſs. Proceeding, however, in this manner, I found that nothing came over in the form of *ſteam*, but that there was a great quantity of *air*, ſeveral hundred times more than the bulk of the water, and at that time there was in it a conſiderable proportion of fixed air, which I imagined might either be that which had not been ſufficiently expelled before, or might be compoſed of ſome phlogiſtic matter contained in the lime, and the purer air that was yielded by the water: for I own I then concluded, that the air which I got (and which, when the fixed air was extracted from it, was ſuch as a candle would juſt burn in) came from the water, eſpecially as in ſome of the proceſſes, the weight of the air I caught was very nearly, if not quite equal to that of the water, and interpoſing a large glaſs balloon between the retort and the recipient for the air, I obſerved that it remained perfectly cool and dry during the whole proceſs; and ſeveral hours afterwards there was not the leaſt moiſture condenſed in it. I alſo received a quantity of another produce of air made in this manner in mercury, and having viewed it with the greateſt attention, obſerved that, after ſeveral days, it never depoſited the leaſt moiſture.

I then calcined a quantity of natural lime-ſtone with this glaſs balloon, interpoſed in the ſame manner, and found no water, but only air to come from it, though the ſtone is generally ſuppoſed to contain water. But when I uſed much more than half an ounce of water to the quantity of whiting or lime above-mentioned, I always had ſome water come over, though very little in proportion to the quantity made uſe of.

I did not fail to examine whether there had been any loſs in the weight of the lime, or whiting, in order to determine whether any part of theſe ſolid ſubſtances had entered into the compoſition of the air; but I found much difficulty in weighing them with exactneſs, after ſhaking them out of an earthen retort, into which I could not ſee, and to which part of theſe earthy matters often adhered, ſo that I could not obtain much ſatisfaction even when I broke the retort. Beſides, there was always ſome loſs of the earth in the cloudineſs of the air, whenever the production of it was rapid. In a future proceſs I had abundant proof that the air did not come from any earthy matter with which the water had been combined.

Hitherto I had no idea but that all that was neceſſary to the converſion, as I concluded it to be, of water into air, was to give it a red heat, without which it would not quit the calcareous earth; and I imagined by this means the matter or principle of *heat* was ſo intimately combined with it, as not to be ſeparated from it by cooling, as in the caſe of ſteam. But I, as well as all my friends, was a long time utterly diſconcerted upon finding that when I put the whiting and water into a coated glaſs retort, the water came over in the form of ſteam, and little or no air was produced. The reſult was alſo the ſame when I made the proceſs in a gun-barrel, in a porcelain retort, or even in an earthen retort glazed in the inſide.

That the earth had not loſt its property of doing its part in the buſineſs, I found by putting more water to the ſame whiting which had failed in the glaſs retort, and which had been uſed no leſs than four times before, and then heating it in an earthen retort; when again it gave air only, and no water, the ſame as before. And at this time I obſerved, that part of the air was hardly to be diſtinguiſhed from that of the atmoſphere.

I cannot expreſs my ſurpriſe at my unexpected failure with the glaſs retort; and my ſpeculations on the ſubject were various, but at that time altogether ineffectual. Among other things it occurred to me that, poſſibly, ſome phlogiſton, either contained in the earthen retort, or coming through it (though I could not tell how, or on what principle) from the fire, might be neceſſary to water, and all other ſubſtances, aſſuming the form of air. But when, with this idea, I put ſpirit of wine, oil, or iron-filings to the lime, I got nothing from theſe mixtures in glaſs retorts beſides ſteam and inflammable air, from the decompoſition of theſe ſubſtances containing phlogiſton.

That there was nothing in the materials of which the earthern retort was made that neceſſarily produced the air, was evident from my not ſucceeding when I pounded a broken retort, and heated it, mixed with water, in one of glaſs.

Being ſatisfied that the production of air depended very much upon the retort itſelf, I thought of uſing the retort only with water, but without any lime, or earthy ſubſtance; and I found it ſucceed far beyond my expectation. For when I put a ſmall quantity of water into one of theſe retorts, and endeavoured to diſtil it gently, I never failed to procure about an hundred ounce meaſures of air; and this I could do as often as I pleaſed, with the ſame retort, and without its loſing any weight; and the air produced in this manner had never any portion of fixed air in it, and was always but very little inferior to that of the atmoſphere.

In all theſe proceſſes I obſerved, that very little of this air was procured till all the water that could be poured out of the retort was evaporated, for the difference in the produce was very little, whether I expoſed the retort to the fire quite full of water, or with only about an ounce meaſure of water in it, or even after letting it remain full for a ſhort time, and then pouring out all that I could from it; ſo that it was only that water which was entangled, as it were, in the pores of the retort, and which had been in ſome meaſure united to the ſubſtance of it, that had contributed to this production of air.

Theſe retorts (which Mr. WEDGEWOOD informs me are made of a mixture of freſh and of burnt Devonſhire pipe clay) are pervious to water, though not to air; ſo that while the air is produced from that water which has entered the pores, the reſt is ſometimes viſibly making its eſcape in the form of a copious ſmoke on the outſide. It was evidently impoſſible, however, and contrary to all the laws of hydroſtatics, that air ſhould enter by the ſame pores by which the water or ſteam was eſcaping, and at the ſame time that its endeavour to force its way out of the retort was ſuch that it overcame a conſiderable reſiſtance from the column of water, at the mouth of my recipients. Air might have *eſcaped* through any unobſerved pores in the retort, but none could have *entered* that way: and if there was the leaſt ſenſible crack in any part of the retort, I was never able to collect any air at all.

But the following experiments may, perhaps, ſhew that it is ſufficient for the production of air that ſteam come into contact with clay ſufficiently heated. Between a copper ſtill and the glaſs tube communicating with my recipient for air, I introduced the ſtem of a tobacco-pipe; and by means of a ſmall furnace, I kept about three inches of the middle part of it moderately red-hot. In this ſtate, making the water boil, I uniformly received air, though mixed with ſteam, at the rate of five ounce meaſures in twelve minutes for more than an hour; but when I let the pipe cool, nothing but ſteam was delivered by it without any air at all. There was no fixed air in this produce, and it was all ſuch as a candle would hardly have burned it. It might, I thought, have been better and alſo more in quantity if I had not uſed the ſtem of a foul pipe. But when I uſed a clean pipe in the ſame manner, I did not find the air much, if at all improved. Suſpecting this to ariſe from the near contact of the fuel, I incloſed the tobacco-pipe in an earthen tube, and then I had air as good as I had generally got in the earthen retort, and not much worſe than that of the atmoſphere.

Another circumſtance I obſerved was, that if the outſide of the veſſel which contained the water or ſteam, through which it paſſed, when the requiſite heat was applied to it, was not dry, or perhaps ſurrounded with good air (for in thoſe circumſtances the following experiment differs from the preceding ones) the experiment did not ſucceed.

When I put the ball of an earthern retort, filled with moiſt clay, into an iron digeſter, and applied heat to it, I get only a very little fixed air, which was probably compoſed of a ſmall quantity of air beginning to be produced from the materials and inflammable air from the veſſel. All that come over beſides was ſteam, and at laſt inflammable air, from the veſſel itſelf.

Being now able to procure air by means of water in this moſt ſimple method, *viz.* by water only in the earthen retort, I had an opportunity of aſcertaining, with great eaſe and exactneſs, ſeveral circumſtances relating to the proceſs, and of obviating, as I thought, ſome objections to the concluſion that I had drawn from it. Among other things I fully ſatisfied myſelf that the *earth of the retort* contributed nothing at all to this production of air, but the *water only*: for having uſed the ſame retort till I had got from it nearly an ounce weight of air, or 800 ounce meaſures, I found that it had not loſt ſo much as a ſingle grain in weight. After the firſt proceſs it weighed juſt three grains more than it did at firſt, and it continued to weigh the ſame till after the laſt proceſs. This ſmall addition of weight might eaſily have come from a little of the water having been imbibed by the neck of the retort, where the heat of the fire could not reach it. When all the proceſſes were over, I kept the whole retort in a red heat for ſeveral hours, and then found that, beſides loſing thoſe three grains, it weighed eight grains leſs than it did at firſt.

Before this I had found, that the calcined whiting which I had uſed in the firſt experiment could not, as ſome ſuppoſed, attract from the atmoſphere any conſiderable part of the air which I got from it, after combining water with it: for two ounces of the whiting (which was the quantity which I generally made uſe of) did not attract more than eight grains of any thing when it was expoſed a whole day in an open diſh, though it had loſt more than half its weight in calcination.

It has been imagined by ſome, that the air which I got in theſe earthen retorts was that which had been attracted from the atmoſphere by the inſide ſurface of them. But, beſides that no air could ever be produced without water, to obviate this objection more particularly, when one of theſe retorts was giving its laſt portion of air, I immerſed the mouth of it in a baſon of water; and letting it cool in that ſituation, filled it again without admitting any acceſs of air to the inſide; and yet, on repeating the proceſs with it, the air was produced juſt as freely as before. This operation I repeated ſeveral times. If it be ſaid, that the outſide of the retort attracted the air, ſtill the inſide, being compoſed of the ſame materials, muſt have attracted air alſo; and it would have appeared by the aſcent of the water from the baſon, the retort being ſufficiently impervious to air.

By ſome it was imagined, that either the air itſelf that I procured, or at leaſt the power of the retort to contribute to the production of it, was owing to ſomething that was tranſmitted from the burning coals, but which could not paſs through glaſs or metals. To determine this, I took an earthen tube, of the ſame compoſition with the retort, and putting a little water in it, placed it, ſurrounded with ſand, in a glaſs veſſel, and this again, ſurrounded alſo with ſand, in an iron one; and yet the heat tranſmitted through all theſe ſubſtances enabled the earthen tube to give air, in the ſame proportion, and of the ſame quality, as it would have done if it had been expoſed to the naked fire.

Having now procured air, by means of a water, in a very ſimple and, as I thought, an unexceptionable manner, I wiſhed to make it in greater quantities in proportion to the water employed; and for this purpoſe I firſt thought of increaſing the ſize or the thickneſs of the porous retorts; but I thought it might anſwer as well if I put into the retort, in powder, the materials of which they were made, or other ſubſtances of the ſame kind.

Accordingly, by mixing ground flint and clay in various proportions, I preſently increaſed the quantity of air much beyond my expectation. In the firſt trials, in which I had much flint and a little clay, I never failed to get 200 ounce meaſures of air from one of water. Then, uſing more clay and leſs flint, I had ſtill more air; and at laſt, leaving out the flint altogether, and uſing clay only, I never failed to get much more than 400, and generally between 500 and 600, ounce meaſures of air from one of water, which was about three-fourths of the weight of the water; and in one particular proceſs I procured very little leſs than nine-tenths of the weight of the water in air, and this air was never much leſs pure than that of the atmoſphere. Sometimes it could not be diſtinguiſhed at all from it at all by the teſt of nitrous air; and once or twice I thought it even purer than that of the atmoſphere.

I muſt here obſerve, that I found it not convenient to put ſo much water to any quantity of clay as would make it cohere in one maſs, but only ſo much as that it ſhould remain in the form of powder. By this means it would eaſily pour out of the retort when the experiment was over.

The weight of the water expended in this production of air I aſcertained, in the moſt unexceptionable manner, by weighing the retort, with all its contents, before and after the proceſs. I ſhall explain this by the reſult of two of the proceſſes. In one of them, the retort and moiſtened clay together loſt in weight 1 oz. 4 dwts. 12 grs. after yielding 741 ounce meaſures of air, which (in the proportion of ſix grains to one ounce meaſure) would have weighed 18 dwts. 12 grs. and conſequently three-fourths of the weight of the water.

In the other proceſs the loſs of weight was 15 dwts. 18 grs. after yielding 556 ounce meaſures of air, which would have weighed 13 dwts. 21 grs. The proportion, therefore, between the weight of the air and that of the water was 111 to 116, or nearly nine to ten.

I alſo found now, that ſo much heat as I had hitherto applied was neither neceſſary nor uſeful. In the laſt mentioned proceſs the retort was conſtantly ſuſpended about ſix inches above a moderate charcoal fire; at another time more than twelve or fifteen inches above it, where a FAHRENHEIT's thermometer did not ſhew more than 210°. With this moderate heat I got 465 ounce meaſures in the courſe of about twelve hours. When the retort was ſuſpended within ſix inches of the fire, the air was generally produced at the rate of 30 ounce meaſures in five minutes. But a thermometer, the bulb of which was immerſed in the clay, was ſtill only at the heat of boiling water.

In all theſe proceſſes, however, there was evidently ſome loſs of water; for, excepting the firſt experiment with the lime, I never got the whole weight of the water in air; and it might be ſaid that I only expelled the air before contained in the water, though from theſe experiments it appeared to contain much more air than it had been thought capable of containing. To obviate this objection, I contrived to catch all the water that eſcaped through the pores of the retort in the following manner.

Having put the moiſtened clay in an earthen tube, to which I had fitted a cock and a long glaſs tube (by means of which I could collect all the air that came from it), I put this within an iron tube, which was cloſed at the end next the fire, but open at the other end, and ſo long that I could eaſily keep this open and quite cool while the other was in the fire; conſequently, whatever water eſcaped through the pores of the earthen tube, it would be condenſed in the cool part of the iron one. This water I carefully collected, and always found that the weight of it, together with that of the air produced in the experiment, was nearly that of the original weight of the water, eſtimated by the loſs of weight in the earthen tube and its contents. I alſo found, that the water ſo collected ſerved for the production of more air, juſt as well as any other water whatever, ſo that there had been no decompoſition of the water in the caſe.

In the laſt proceſs that I went through of this kind, the loſs of weight in the earthen tube, or rather of the water contained in it, was 12 dwts. 4 grs.; the air collected was 173 ounce meaſures, which would have weighed 4 dwts. 3 grs. and the water which eſcaped through the pores of the earthen tube, and which I collected, was nearly 8 dwts. 3 grs.; ſo that the air and this water together weighed 12 dwts, 14 grs. or ten grains more than the original water. But as I eſtimated the weight of the water only by the ſpace which it occupied in a

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cylindrical glaſs tube, divided according to ounces and parts of ounces of water, it was not eaſy to avoid an error of a few grains. At other times there was an error of a ſmall magnitude on the other ſide. But it will appear hereafter, that more ſteam muſt have eſcaped inviſibly at the open mouth of the iron tube than I was aware of.

That nothing could enter by the pores of the retort at the ſame time that the water was making its eſcape out of them, I thought I aſcertained pretty ſatisfactorily by immerſing the bulb of it in mercury, contained in an iron veſſel. In theſe circumſtances I obtained air as uſual, only the produce was not ſo rapid. In this way, however, I procured above an hundred ounce meaſures of air from moiſtened clay; and I diſcontinued the proceſs without perceiving any termination of it. But the moment the retort was raiſed out of the mercury, it gave air three times as faſt as it had done before. The quality of the air was the ſame in both caſes, *viz.* a little worſe than that of the atmoſphere.

I even collected thirty ounce meaſures of air when the bulb of the ſame retort was immerſed in hot linſeed oil, but the production of air gradually ceaſed, and the next day I found the retort almoſt full of the oil, which had ſoaked through it. Diſtilling this oil I get 300 ounce meaſures of air, wholly inflammable, except a very few ounce meaſures at the laſt, which were only phlogiſticated.

Still hearing of many objections to the converſion of water into air, I now gave particular attention to an experiment of Mr. CAVENDISH's, concerning the re-converſion of air into water, by decompoſing it in conjunction with inflammable air. And in the firſt place, in order to be ſure that the water I might find in the air was really a conſtituent part of it, and not what it might have imbibed after its formation, I made a quantity of both dephlogiſticated and inflammable air in ſuch a manner as that neither of them ſhould ever come into contact with water, receiving them as they were produced in mercury; the former from nitre, and in the middle of the proceſs (long after the water of cryſtallization was come over), and the latter from perfectly-made charcoal. The two kinds of air thus produced I decompoſed by firing them together by the electric exploſion, and found a manifeſt depoſition of water, and to appearance in the ſame quantity as if both the kinds of air had been previouſly confined by water.

In order to judge more accurately of the quantity of water ſo depoſited, and to compare it with the weight of the air decompoſed, I carefully weighed a piece of filtering paper, and then having wiped with it all the inſide of the glaſs veſſel in which the air had been decompoſed, weighed it again, and I always found, as nearly as I could judge, the weight of the decompoſed air in the moiſture acquired by the paper.

As there is a ſource of deception in this experiment, in the ſmall globules of mercury, which are apt to adhere to the inſide of the glaſs veſſel, and to be taken up by the paper with which it is wiped, I ſometimes weighed the paper with the moiſture and the mercury adhering to it; and then expoſing it in a warm place, where the water would evaporate, but not the mercury, weighed it again, and ſtill found, as nearly as I could pretend to weigh ſo ſmall a matter, a loſs of weight equal to that of the air. I wiſhed, however, to have had a nicer balance for the purpoſe: the reſult was ſuch as to afford a ſtrong preſumption that the air was re-converted into water, and therefore that the origin of it had been water.

Another preſumption in favour of the generation of our atmoſphere from water was, that the purity of the air that I produced from it is ſo very nearly the ſame with that of the atmoſphere. And the degree of heat requiſite to produce it is no greater than may be given by the rays of the ſun in certain circumſtances. Subterraneous fires, however, would be abundantly ſufficient for the purpoſe, as it appears to be ſufficient for the converſion of water into reſpirable air, that it come into contact with clay, and perhaps many other earthy ſubſtances in the form of vapour. I muſt, however, obſerve, that when I threw the focus of a burning lens upon a quantity of moiſt clay, either *in vacuo*, or in common air, I got no air from it.

I made this experiment both with the clay expoſed in an open diſh, and alſo confined in a ſhort earthen tube. Had I then proceeded to repeat this laſt proceſs with a communication between the inſide of the earthen tube and the external air, as I then propoſed to do, but was prevented, I ſhould much ſooner have diſcovered what I did afterwards, *viz.* that there was no real converſion of water into air in this proceſs. In favour of which, however, it may not be amiſs to obſerve, that the great difficulty Mr. DE LUC and others have found in expelling all air from water, is beſt accounted for on the ſuppoſition of the generation of air from water, though in other circumſtances than thoſe that I have obſerved. I have the pleaſure to add, that Mr. DE LUC himſelf concurs with me in this opinion.

The difficulty that ſtrikes many perſons the moſt forcibly, is the want of analogy between the converſion of water into air with any other known facts in philoſophy or in nature. But admitting that this converſion is effected by the intimate union of what is called the *principle of heat* with the water, it appears to me to be ſufficiently analogous to other changes, or rather combinations of ſubſtances. Is not the acid of nitre, and alſo that of vitriol, a thing as unlike to air as water is, their properties being as remarkably different? And yet it is demonſtrable, that the acid of nitre is convertible into the pureſt reſpirable air, and probably by the union of the ſame principle of heat.

It is true, that ſteam is a thing very different from air, and I find that it is not able to decompoſe nitrous air; but then, though it has acquired ſenſible heat, it has got no latent heat ſo intimately combined with it as it is with air; and for the ſame reaſon, perhaps, the vapour of nitrous acid is not dephlogiſticated air.

By the ſame proceſs by which reſpirable air is made by means of water, inflammable air may be made from liquid ſubſtances containing phlogiſton. Making ſpirit of wine to boil in a glaſs retort, I made the vapour paſs through the ſtem of a hot tobacco-pipe, and found that it was all ſeemingly converted into inflammable air, and it was of that kind which burns with a lambent white flame. But when I let the pipe cool no air was produced, but only vapour, which was inſtantly condenſed in the water.

Being now maſter of a new and eaſy proceſs, I was willing to extend it to other liquid ſubſtances; and I preſently found, as I then imagined, that, by this means, I could give a permanent aërial form to any liquid ſubſtance that had been previouſly thrown into the form of vapour.

When I made the vapour of ſpirit of nitre, heated in a glaſs retort, paſs through the ſtem of the hot tobacco-pipe, I got as pure dephlogiſticated air as ever I have procured from nitre; though the cork, by which the retort was connected with the pipe, was diſſolved, and muſt have contributed to contaminate it, and give it a ſlight mixture of fixed air.

With oil of vitriol I got air conſiderably phlogiſticated, ſo that a candle would not have burned in it; but this I alſo attribute to the cork, which was diſſolved in the proceſs. The reſult was nearly the ſame when I uſed water impregnated with vitriolic acid air, though the cork was not diſſolved. But this acid is known to contain much phlogiſton.

Spirit of ſalt gave air no purer than the beſt atmoſpherical air. But as by this proceſs I never got air ſo pure as this from water only, I concluded, that even this acid, as well as the nitrous and vitriolic, is capable of being turned into dephlogiſticated air.

When I uſed water impregnated with fixed air, this air was expelled by the heat, and came over without any change that I could perceive, except that the reſiduum was larger, from the water that came along with it. The air I got afterwards was only that from the water, and of the ſame quality as if it had not been impregnated with fixed air.

Water impregnated with alkaline air gave neither fixed nor inflammable air, which I had rather expected, but only air conſiderably phlogiſticated; though ſome of it was ſo pure that a candle would have burned in it.

N. B. In all theſe experiments with the tobacco-pipe all the air was remarkably turbid, like milk, and even the common air in the retort before the proceſs properly began.

In this ſtate of the experiments I think I may venture to ſay, that no perſon could have ſeen them without concluding that there was a real converſion of water into air, there being no known principle or fact in philoſophy, that could have led any perſon to ſuſpect a fallacy in the caſe. In this, therefore, I muſt have acquieſced, as indeed did all my acquaintance, even thoſe who had been the moſt incredulous on the ſubject, after they had themſelves ſeen the experiments. But I was led to the farther proſecution of this buſineſs, in conſequence of having obſerved that the *purity* of the air which I procured depended upon the ſtate of that which was immediately contiguous to the earthen retort, or tube, in which I ſuppoſed the converſion to have been made; and that ſome communication with the atmoſphere was neceſſary to the production of any air, as in the experiment with the digeſter, and thoſe with the clay and the burning lens. And ſince pure external air was neceſſary in order to procure good air, it was concluded by ſeveral of my friends, and eſpecially Mr. WATT, that the operation of the earthen retort was, to tranſmit phlogiſton from the water contained in the clay to the external air; and that the water, thus dephlogiſticated, was capable of being converted into reſpirable air by the intimate union of the principle of heat.

In order to aſcertain what the influence of the external air in this caſe really was, I incloſed an earthen retort filled with moiſtened clay in a large glaſs receiver, open at both ends, through the upper orifice of which (being narrow) I thruſt the neck of the retort, luting it ſo as to be perfectly air-tight; and placing the receiver in a baſon of water, by which the air within was cut off from all communication with the external air, I fitted to the mouth of the retort a glaſs tube, through which I could receive whatever was produced in the proceſs. In this ſituation I heated the retort by means of Mr. PARKER's excellent burning lens, when air was received through the tube communicating with the inſide of the retort as uſual; but at the ſame

time

time the water roſe within the receiver. This effect might be owing to a phlogiſtication of the air within the receiver; but it was ſoon diminiſhed far beyond the utmoſt limit of that proceſs, ſo that very little of it remained; and examining this air, I found it to be but very little worſe than that of the atmoſphere, as that which came from the retort was a little better.

This experiment made it probable, that the air on the outſide of the receiver had actually paſſed through it, only a little purified in its paſſage; and yet it was contrary to all the known principles of hydroſtatics, and even any thing hitherto known in chemiſtry, that air ſhould be tranſmitted through a veſſel of this kind, and in a direction contrary to that in which it would have been forced by the preſſure of the atmoſphere; while the water, with which the clay was moiſtened, went the other way. For had the retort been pervious to air, as the inſide had a free communication with the atmoſphere, the water could not have riſen within the receiver. This, however, appeared to be the caſe by the following deciſive experiments.

Having filled the earthen retort with the moiſtened clay as before, I made the inſide of the receiver perfectly dry, and placed it in a baſon of mercury; when, upon heating the retort as before, the receiver was all covered with dew, which collecting into drops trickled down the inſide of the receiver, and remained upon the mercury, which roſe within the receiver, while air was received from the retort as uſual. I had no doubt, therefore, but that all the water within the retort would have got through into the receiver. Spirit of wine, or ſomething that had the ſmell of it, was tranſmitted from the clay through the retort in the ſame manner.

I then filled the receiver with inflammable air, and upon heating the retort it was all drawn through it, and delivered as ſtrongly inflammable as ever by the tube communicating with the inſide of it, while the water roſe within the receiver, and even covered the retort, which was fixed at the very top of it, ſo that hardly any of the inflammable air remained within it. In like manner nitrous air paſſed through the retort unchanged.

From theſe experiments it is impoſſible not to infer, that the clay of the earthen retort, being thus heated, deſtroys for a time the aërial form of whatever air is expoſed to the outſide of it; which aërial form it recovers after it has been tranſmitted in combination from one part of the clay to another, till it has reached the inſide of the retort, while the water is drawn through it in the contrary direction.

Had this hypotheſis been propoſed *a priori*, it would, I doubt not, have been thought more extraordinary than the converſion of water into air. I propoſe to make many other experiments in the proſecution of theſe; but till I have an opportunity of doing this, I ſhall not trouble the Society with any conjectures that have occurred to me on the ſubject.

The great difficulty with reſpect to the experiment with the lens is, that the water ſhould paſs through the retort one way, and the air the other, and yet that the air ſhould not be able to paſs without the water. It is alſo not a little extraordinary, that the weight of the air and that of the water ſhould be ſo nearly equal.

In the laſt place I muſt obſerve, that there is nothing in this experiment that contradicts the idea of the converſion of water into air, though it does not prove it: for ſtill the experiment with the tobacco-pipe, in which the ſteam is made red-hot (whereas in that with the lens it is only of a boiling heat) cannot be explained ſo well on any other hypotheſis any more than Mr. CAVENDISH's experiment on finding water on the decompoſition of air.

I cannot conclude this account without acknowledging my obligation to Mr. PARKER for the uſe of his incomparable lens, and his obligingly aſſiſting me in the management of it. Indeed, without this very inſtrument, or one of greater power than my own, I do not know that the laſt mentioned experiments could have been made at all; certainly not to ſo much ſatisfaction.